



From the desk of

KEN BIEGELEISEN

7/18/77

(File)

Dear Dr. Crick:

Thank you very much for your letter of 7/13/77. **I** did not realize that you were being consulted (hopefully not too bothered) by other proponents of alternative structures for DNA. In view of this, I appreciate all-the-more your taking the time to read my letter.

Sincerely yours,
Ken Biegeleisen

Biegeleisen

July 13, 1977

Mr. Ken Biegeleisen
340 East 34th Street
New York, NY 10016

Dear Mr. Biegeleisen:

My apologies for not replying sooner to your letter of May 11th. I enclose a copy of a letter I wrote on May 11th to Dr. Rodley which covers much the same ground as your letter. While at Cold Spring Harbor I discussed the problem with both Mike Botchan and Walter Keller. They believe that the present experimental evidence on D-Loops rules out any SBS structure and shows that there are about ten base-pairs/burn. However, as we now need to know this number very accurately, they propose to carry out improved experiments, perhaps later this year.

In summary, I think it likely that we can dismiss any SBS (\neq TN) model for normal DNA. Whether these are any circumstances in which such a structure could form is another matter.

Yours sincerely,

F. H. C. Crick
Kieckhefer Distinguished
Research Professor

FHCC/bml

Enclosure

May 25, 1977

Mr. Ken Biegeleisen
340 E. 34 Street
New York, NY 10016

Dear Mr. Biegeleisen:

I am writing to acknowledge and thank you for your letter of May 11 and enclosure to Dr. Crick which was forwarded to him here. Dr. Crick is on the east coast at present and I shall bring these to his attention when he returns to the Institute the first of July.

Yours sincerely,

Mrs. Kathleen Verlander
Secretary to Dr. F.H.C. Crick

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19th May, 1977

Mr. Ken Biegeleisen,
340 E. 34 Street,
New York, N.Y. 10016,
U.S.A.

Dear Mr. Biegeleisen,

I am writing to acknowledge receipt of your letter and enclosure addressed to Dr. Crick.

Unfortunately Dr. Crick has now retired from the Medical Research Council and has taken up an appointment at The Salk Institute, California, and I have therefore forwarded your correspondence on to him there.

Yours sincerely,

S.M. Wilby
Secretary to Dr. F.H.C. Crick

19 MAY 1977

5/11/77

340 E. 34 St.

New York, N.Y. 10016

Dr. F.H.C. Crick

M.R.C. Unit for the study of the Molecular
Structure of Biological Systems
The Cavendish Laboratory
Cambridge, England

Dear Sir:

Five years ago, while a devout student of science at Cornell University, I read "The Double Helix" and immediately converted to molecular biology. I became determined to make a major contribution. To this end I worked and studied furiously, 19 hours per day, 7 days a week, for over 3 years. Very early in this course, I became somewhat possessed by the peculiar idea that the strands of circular DNA are topologically non-linked, and can therefore be separated with both strands intact, if only someone would take the trouble to find the conditions under which such separation would occur.

At the time, I discussed this idea with Dr. Richard Hallberg, a former student of J. Vinograd. Hallberg was very much entertained, but not at all impressed. Since then, every circular DNA worker with whom I have discussed this idea has reacted similarly. To get these people to seriously consider this idea is as impossible as getting oil to

1977 JAN 8
dissolve in water. And yet, the various discoveries in circular DNA have not, in 5 years' time, excluded this idea, and may in fact have advanced it.

I attempted to approach this problem experimentally, but my current Ph.D. mentor (another Vinograd disciple) would not approve of such a venture. When it became obvious that I would not, in the foreseeable future, be able to do any of the necessary experiments, I wrote up the theory of topologically non-linked DNA and submitted it to the J. of Theoret. Biol. It was, not unexpectedly, resoundingly rejected. I then sent copies of the rejected article to Pohl and Rodley (see Refs. 7 and 10), the two investigators most likely (in the whole world, I would think) to appreciate this theory, since their work supports its feasibility. They have not responded.

Since this theory started (via "The Double Helix" and therefore through no fault of your own) with you, I think it appropriate that it should end with you. I cannot shake the feeling that someday, perhaps 50 years from now, some molecular biologist will inadvertently incubate a form I DNA under the proper strand-separating conditions, and unexpectedly find himself with a pure preparation of intact, single-stranded circles. I would be deeply grateful if you would write me a letter telling me that you think topologically non-linked circular DNA is at least possible, so I have something to show my children on that fateful day 50 years from now, to prove to them that once in my life I had a good idea.

Yours truly, Ken Biegelisen